

CORRESPONDENCE

S-T

24 (23)

Box 27 Folder 23

1949-1951



Correspondence S-T

- ① Letter of introduction for MacNeish, from Gabriel Saldívar, México D.F., to Hugo Pedro González, Governor of Tamaulipas, 12/8/45  
List of flight times on reverse. (See also Content #36.)
- ② Letter from MacNeish to Walter W. Taylor, 5/21/51, requesting information on Gahuila
- ③ Letters between MacNeish & Taylor: 9/11/57, Taylor asking for reprints; 9/24/57 - MacNeish's response; 12/10/56 - MacNeish requesting exchange of information
- ④ Letters between MacNeish & J. Eric S. Thompson, exchanging information. Thompson's letters & cards are hand written.
- ⑤ Letters between MacNeish and Raymond H. Thompson, editor of American Antiquity: 4/17/57 - Request for MacNeish to review The Abbot Farm, by Dorothy Cross; 11/21/57 - Review of The Abbot Farm by MacNeish; 12/8/57 - Am. Antiquity's receipt of review; 12/17/57 - MacNeish sending Tamaulipas reprints; 1/6/58 - receipt of reprints
- ⑥ Letters between MacNeish & Raymond H. Thompson concerning Taylor's review of MacNeish's Sierra de Tamaulipas report, MacNeish's response, & Taylor's reply. Includes MacNeish's original last page with Thompson's editing. See also Content # 11.
- ⑦ Letter from Paul Tolstoy to MacNeish, 1/16/61, asking for Tamaulipas reprints; 1/19/61 - secretary's response
- ⑧ Letters from MacNeish to A.M. Tozzer, Peabody Museum, Harvard, requesting information on Maya figurines, 1/22/51 and 3/10/51. No response.
- ⑨ Letters between MacNeish and Rudy C. Troike, U. of Texas, about Tamaulipas reprints, 5/12 + 5/19/58
- ⑩ Letter from MacNeish to Mr. Turner about sites on the Nehanni R., Northwest Territories, 12/22/49
- ⑪ Letter from MacNeish to Dick Turner about Liard-Netla River area, with comment about wife's (June Helm) study of Sladé, 2/23/51



Gabriel Saldívar.  
Anáhuac Núm. 86  
Col. Roma Sur.  
Mexico, D.F.

México, D.F., a 8 de diciembre de 1945

Sr. Lic. Hugo Pedro González.  
Gobernador del Estado.  
Palacio de Gobierno.  
Ciudad Victoria, Tamps.

Muy estimado y fino amigo:

Esta carta le será presentada por el Dr. R.S. Mac Neish, Arqueólogo que tiene permiso del Instituto Nacional de Antropología para hacer exploraciones de su especialidad y localización de vestigios de la prehistoria de nuestro Estado.

Me ha sido presentado por don Enrique Juan Palacios, funcionario del mencionado Instituto y por el Dr. Gordon F. Ekholm, Arqueólogo que hizo investigaciones en Tamaulipas.

Como las investigaciones del Dr. Mac Neish redundarán en grande beneficio para el conocimiento de la Historia de nuestros primitivos antepasados y sus relaciones con las culturas vecinas, tanto de la Huasteca como del Sur de Texas, me permito presentarlo a usted con la súplica de que se le den las facilidades posibles, presentándole con las autoridades locales y personas que puedan darle informaciones sobre la materia.

Los saludo muy afectuosamente y me repito su atento amigo y seguro servidor

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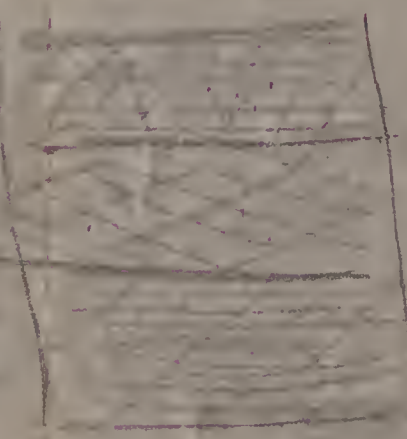
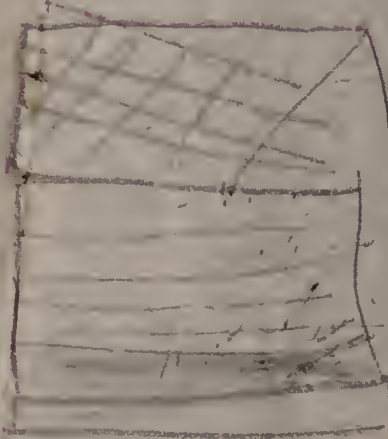
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copy

May 21, 1951.

Dr. Walter W. Taylor,  
711 Camino del Monte Sol,  
Santa Fe, New Mexico, U.S.A.

Dear Walt:

Since I last saw you in Santa Fe at the archaeological meeting three years ago I have done considerable more work in Tamaulipas much of it in caves like yours from Coahuila. Since much of my analysis is now completed and a series of foci are discernable, I am looking for comparable material. Therefore, I was wondering if it would be possible to please have information about what cultures occurred in your caves and what are the sequences of artifacts. I would be most willing to send you similar information from Tamaulipas.

Are you planning to do more work in Mexico?

Give my regards to everybody around Santa Fe.

Sincerely yours,

Richard S. MacNeish.

1911

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1911

WALTER W. TAYLOR

5 DE FEBRERO NO. 42  
COYOACAN, D. F.  
MEXICO

11 Sept., 1957.

Dr. R. S. MacNeish,  
National Museum of Canada,  
Ottawa, Ontario.

Dear Scotty:-

Just a note to follow up what you told me to do while we were in Chicago last March.....not really much later than I get around to most things!

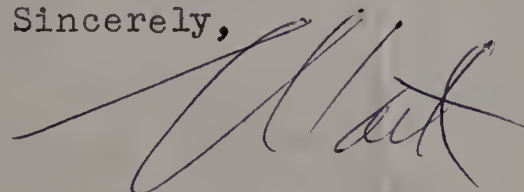
You said that if I would write, your secretary (please note!) would send me whatever reprints you have on your archaeological work in northeastern Mexico, particularly Tamaulipas. Well, here I am writing.

Anything you care to pass on will be well used, both by myself and my students, the which are coming to the house in ever increasing numbers to use the library.

In passing, I might say that the editing of our Chicago meeting Proceedings is coming....but slowly. In fact, I do not yet have all the papers, but I guess I have all I'm going to get, so I've begun to edit. I expect I will do a great deal of cutting and piecing, because as was to be expected the articles are very uneven and, without much editing, will not give anything like the picture that came out of our meeting.

Take care, and I suppose I'll be seeing you in Chicago this Christmas.

Sincerely,







XXXXXXXXXXXXX  
XXXXX

Human History Branch

Ottawa, September 24, 1957.

Mr. Walt W. Taylor,  
5 De Febrero No. 42,  
Coyoacan, D. F.,  
Mexico.

Dear Walt:

Having just got back from the Yukon, your letter from Mexico seems like it comes from another world. I am enclosing a number of reprints on assorted subjects concerning northeastern Mexico with this letter. Unfortunately, two of the more enlightening articles on the area are without reprints, one is from the Revista Mexicana and the other from Willey's settlement patterns in the New World, but I suspect you have these already.

Have been trying to get a conference going with botanists, geologists, and so forth, on northeastern Mexico, with an eye to identifications and ecology. As yet, I have not been too successful but will get back at it now that I am back from the field.

Don't know when I'll get back to Mexico but hope to see you in Chicago at Christmas.

By the way, my larger volume entitled, "A Preliminary Report on the Archaeological Sequences in the Sierra de Tamaulipas, Mexico" was submitted to the American Philosophical Society last spring. So far they have told me the reader has approved of it and they are now awaiting the decision of the Publications Committee. Please burn a small candle for me in some Mexican cathedral.

Sincerely yours,

Enclosures

Richard S. MacNeish





Ottawa, December 10, 1956.

Mr. Walt Taylor,  
Escuela de Antropologia,  
Museo Nacional,  
Moneda 13,  
Mexico D.F., Mexico.

Dear Walt:

I see by the Christmas meetings that you're giving a paper on the significance of your Carbon 14 dates from Coahuila. Recently I received about a dozen dates on my caves from Tamaulipas and they are all a bit surprising in terms of agriculture in the New World. I am enclosing with this letter a list of the dates and some of their significance.

I have now completely re-written my Sierra de Tamaulipas report and submitted it for publication to the American Philosophical Society and I am about to go to work on the southwest Tamaulipas cave material which had all the nice preservation. In light of the dates you have and the dates I have, it seems like a good idea for me to make some sort of comparisons between materials from Coahuila and Tamaulipas. I wonder how you'd feel about exchanging some information. At present I am working on the projectile points and I only vaguely remember the kinds you have but, as I remember, there were a number of similarities. Would you be willing to trade a few pictures and some description of your points for my descriptions and my pictures of my point types?

Don't know when I'll get back to Mexico City again but may see you one of these days on my way through for Guatemala or Honduras where I may do some work in some dry caves.





Mr. Taylor,

Ottawa, December 10, 1956.

- 2 -

Give my regards to all my pals around the museum and, if you want to, why it's perfectly all right to show them my dates.

Sincerely yours,

Richard S. MacNeish,  
National Museum,  
Ottawa, Canada.

Enclosure

a.



Ottawa, June 3, 1954.

Dr. J. Eric Thompson,  
Harvard,  
Massachusetts.

Dear Dr. Thompson:

Dr. Ekholm suggested that I send you a copy of the final field report for the 3rd Tamaulipas Archaeological Expedition. He felt that you could extract what you see fit for Notes and news. The datings on the pre-La Perra materials is perhaps a little extreme but the sequence is correct.

Sincerely yours,

Dr. Richard S. MacNeish,  
National Museum,  
Ottawa, Canada.

Enclosure

a.

October, June 2, 1954

Dr. J. H. Thompson,  
Biology  
University of Toronto

Dear Dr. Thompson:

Dr. Kihlstrom suggested that I send you  
copy of the final field report for the bird  
surveying archeological expedition. He felt  
that you could extract what you see fit for  
and use. The notes on the pre-1914 birds  
is perhaps a little extensive but the responses  
correct.

Sincerely yours,

Dr. Richard B. MacNeider,  
National Museum,  
Ottawa, Canada

Enclosure



Old Littleton Rd.

Harvard, Mass.

June 7, 1954

Dear Dr MacNeish:

Very many thanks  
for your letter and for the final field  
report, which I was very glad to get.

I wonder whether you would be willing  
to do me a favor. I've been laid up  
for some time, and am under doctor's orders  
to lay off work as much as possible. There  
is quite a lot of material to collate for  
the ~~the~~ next "Notes and News," which  
is due late this month. Accordingly, I  
wondered whether you would be willing  
to make an abstract of your report of  
200-250 words that I could use. Apart  
from the fact that it is hard to type in  
bed, you could abstract the high lights  
much better than I could because you  
have all the material weighed according  
to its significance. Would you be  
willing to do that?

My congratulations on your outstanding  
discoveries

Sincerely Yours  
J. Eric Thompson.

OVER



Would you like the carbon returned?

To: American Antiquity for Notes and News

About: Third Tamaulipas Archaeological Expedition

The Third Tamaulipas Expedition commenced fieldwork in southern Tamaulipas, Mex. on Dec. 1, 1953 and completed its season May 1, 1954. The research was sponsored by the American Philosophical Society, the American Academy of Arts and Sciences, the Botanical Museum of Harvard University and the National Museum of Canada. Dr. R.S. MacNeish was assisted by his wife June, David Kelley, Peter Pratt and Peter Grant.

Reconnaissance yielded 129 sites. Five of these sites were excavated. Two minor digs occurred in the southern part of the Sierra de Tamaulipas which seem to confirm the sequence previously established and added a new cultural complex.

In the Sierra Madre three caves were dug and two were more briefly tested. The first cave, Tm c 247, contained 26 stratified layers with 16 occupation layers containing preserved vegetable materials. The second cave excavated (Tm c 248) had seven occupations with preserved flora. In the third cave (Tm c 274) there was about 6 feet of refuse, and many small stratified occupational lenses. One of the upper lenses contained preserved vegetable remains. As yet, most of the data has not been analysed, but a cursory sorting of the materials revealed a sequence of about seven artifact complexes with distinctive subsistence patterns. In brief, the sequence is as follows (from early to late):

1. Infiernillo complex: Wild foods and some hunting; large choppers, scrapers, and ~~some~~ leaf-shaped points.
2. Portales: Mainly foodgathering, but squash and beans occur; large triangular points, crude scrapers and choppers.
3. Guerra: Mainly foodgathering, but squash, beans, gourds and early Bat Cave type maize present; triangular, leaf-shaped and contracting stemmed points with small scrapers, etc.  
agricultural, with
4. Mesa de Guaje: Mainly/maize (Bat Cave and hybrid races), beans, squash, ~~xx~~ cotton and teocinte present, but some foodgathering; stemmed and notched ~~xx~~ atlatl points with late Formative period pottery.
5. Palmillas: Predominantly agricultural, with hybrid corn races, squash, beans, gourds, cotton, tobacco and chiles; serrated and stemmed points with engraved pottery and pipes (similar to Ekholm's Zaquil Period (IV) from Panuco).
6. San Lorenzo: Half agricultural, half-foodgathering economy; notched arrow points and corrugated pottery.
7. San Antonio: Foodgathering and agriculture; triangular arrow points and crude brushed pottery.





XXXXXXXXXXXXX  
XXXXXX

Human History Branch

Ottawa, June 3, 1957.

Dr. J. Eric Thompson,  
Harvard,  
Massachusetts.

Dear Eric:

Thank you very much for sending me all those publications which arrived this morning. As yet, I haven't had a chance to read them, but I am sure they will be most valuable. In about two hours I will be leaving for the Great Northwest, so I'll have lots of time in the long summer evenings to look them over.

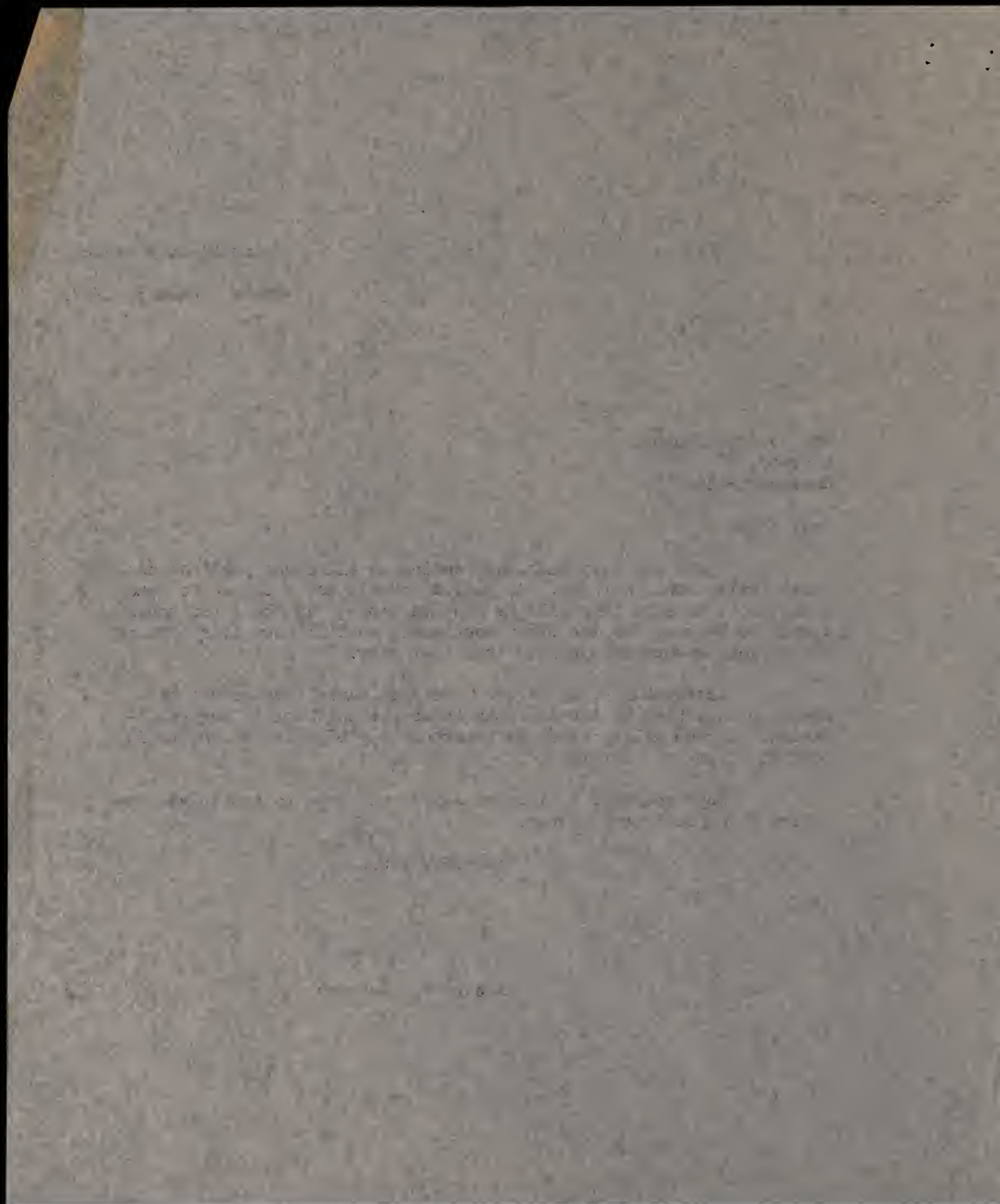
Incidentally, my larger Sierra de Tamaulipas report has now been submitted to the American Philosophical Society for publication. Gordon Willey also has a carbon copy of it which you could look at if you are interested.

Hope you have a pleasant summer and hope to get to see you before I go south next winter.

Sincerely yours,

Richard S. MacNeish

a.





POST CARD



Dr Richard S. MacNeish,  
National Museum of Canada  
(Human History)  
Ottawa,  
Canada



Dr Richard S. MacNeish,  
National Museum of Canada  
Ottawa, Ont.  
Canada.

First fold here

Second fold here

Sender's name and address:

J. Eric S. Thompson  
Ashdon, Saffron Walden,  
Essex, England.

AN AIR LETTER SHOULD NOT CONTAIN ANY  
ENCLOSURE; IF IT DOES IT WILL BE SURCHARGED  
OR SENT BY ORDINARY MAIL.

To open cut here

New address:

NOTE in J. Eric S. Thompson.

HARVARD  
ASHDON  
SAFFRON WALDEN, ESSEX  
ENGLAND

donde siempre tiene la casa, & where we hope to  
hear of your doings. I hear you were entertained by  
Don & Joanna, and hope that doesn't mean you  
are going to become one of those sangrientos Peruvianistas,  
the adjective to be taken in Limey sense.  
Hope to see you at San Jose Costa Rica in July

Eric T.

Harvard, Ashdon, Saffron Walden,  
Essex, England April 12, 1959.

Dear Scotty:

We got back a week ago from a  
grand tour which took us from Massachusetts  
to Mérida via Salt Lake City, Tucson &  
Mexico City & from Mérida on to my old  
stamping ground, British Honduras, &  
thence back here via New Orleans,  
New York & S.S. Saxonia to Tilbury.

It was a delightful tour except that both  
Florence & I felt completely exhausted by  
the time we got home. We saw quite a lot  
of Don & Joanna in Cambridge, both of  
them hope to get to Peru in July or August  
on Fulbrights provided they can pass their  
specials. Don takes his later this month,  
& at present is busily developing stomach  
ulcers as he sweats away & with his usual  
pessimism (manifest since about the age of 9  
when he took his first examination) is sure  
he is going to flunk.

On getting back here, I was welcomed  
by a very pleasant & very important  
addition to my library, your Preliminary

Archaeological Investigations in the  
Sierra de Tamaulipas, Mexico. We

haven't got things straight since we re-  
turned, so I haven't had time to read  
the report so far into the attention it clearly  
deserves, and as yet I haven't done much  
more than give it a quick once over, but I  
see that I'm going to have some good  
reading ahead of me.

I was much interested in your probable  
Mississippi sherds (Ireland incised) as  
probably first trade sherds for that area to  
be found in Mexico - it is a pity the identifi-  
cation could not have been a little more  
specific.

Your cave work in N.E. Mexico has  
certainly paid wonderful dividends. Many  
thanks, indeed, for sending me a copy of the  
report.

Are you planning to go to Vienna next  
year for the Congress of Americanist meeting,  
if so we ought to meet either there or  
before or after the meetings in England.

I trust that you haven't turned your  
back on Mexico. Yrs

Eric Thompson

AMERICAN ANTIQUITY

published by The Society for American Archaeology

Raymond H. Thompson  
Associate Editor for Reviews

Department of Anthropology  
University of Arizona  
Tucson, Arizona  
April 17, 1957

Dr. Richard S. MacNeish  
National Museum of Canada  
Department of Mines and Resources  
Ottawa  
Ontario, Canada


Dear MacNeish:

Will you review The Abbott Farm. Dorothy Cross. Archaeology of New Jersey,  
Vol. 2, Archaeological Society of New Jersey and New Jersey State Museum,  
Trenton, 1956. xii + 215 pp., 17 figs., 46 pls., 14 plans, 1 graph. \$7.00  
cloth, \$8.00 de luxe. }

for AMERICAN ANTIQUITY? The review should be less than 1200 words

long and should be submitted by August 1, 1957 Dick Woodbury wrote that  
you were interested in doing this.

Sincerely yours,

  
Raymond H. Thompson





XXXXXXXXXXXXX  
XXXXXXX

Human History Branch

Ottawa, November 21, 1957.

Mr. Raymond H. Thompson,  
Department of Anthropology,  
University of Arizona,  
Tucson, Arizona.

Dear Ray:

I am sorry I couldn't make the deadline of August 1st for this book review but I didn't receive Cross's book here in Ottawa until I was just leaving for the field in the Yukon and of course with the field season and all I couldn't get around to doing an adequate job until now. Please accept my belated apologies. I sincerely hope that it is not too late.

Winter is about to set in up here and I always write to you people in places like Arizona with some envy. Hope to see you at the Christmas meeting.

Sincerely yours,

Richard S. MacNeish

Enclosure

a.





The Abbott Farm. Dorothy Cross. Archaeology of New Jersey, Vol. 2, Archaeological Society of New Jersey and New Jersey State Museum, Trenton, 1956. xii + 215 pp., 17 figs., 46 pls., 14 plans, 1 graph. \$7.00 cloth, \$8.00 de luxe.

This is basically a site report. It is well written, the drawings and cartography are skilfully done, the photographs of both the excavations and artifacts are clear, and the general organization of the book is excellent. For a clear understanding of the book, I believe it is best to divide my discussion into three parts.

The first matter that Dr. Cross deals with concerns the problem of the claims for "Paleolithic" from the Abbott Farm and adjacent areas. In Chapter I, she has a good review of the previous work, disputes, and so-called conclusions from the period of 1872 to 1956. From Cross's lucid account of the background for the site, one can see, like so many disputes concerning interglacial man in the New World, that wishful thinking, opinions, and passions have far outweighed the artifacts, facts, and objective analyses. In the chapter on geology as well as in the Summary and Conclusions, Dr. Cross gives what I think is a fairly final statement concerning the so-called "argillite cultures", the Paleolithic implements, and the interglacial artifacts in the New World as represented in the Trenton gravels. Briefly, her conclusions are the following:

1. There is no evidence of a Paleolithic culture at the site nor did she find any artifacts in the Trenton gravels.
2. The yellow sands overlying the Trenton gravels in which the so-called "argillite culture" occurred were not waterlaid and are not of interglacial time. The yellow sands, in fact, seem to be relatively recent.
3. There is no evidence of an "argillite culture" per se as argillite





tools are made right up to historic times in this area and do not make up a cultural complex of a distinctive time period.

The second part of my discussion concerns the description of the materials she found at the archaeological site. As an introduction to this, there are brief chapters on the excavations and stratigraphy. The site is divisible into two parts: the Lowlands, and the Bluffs. The Lowlands have rather excellent stratigraphy with four superimposed humuses containing evidence of human occupation, which are separated by relatively sterile sands. The Bluff consists of archaeological materials found in varying depths in yellow sands above the gravels. Some of these artifacts are scattered haphazardly through the sands, while others represent more definite occupations or components as they occur in pits, hearths, caches, burials, and other features.

Next the pits, hearths, burials, and caches are described with lists of associated artifacts. One thing that is particularly troublesome about this section is that little or no cultural assignment either in terms of time periods or in terms of focus or phase is given these archaeological features. Certainly each one represents a component of a cultural entity. Though the contents of the pits, hearths, burials, and caches are described, it is unfortunate that some of the abbreviations and pit listings cloud the cultural affiliations of each pit rather than clarify them. For example, in the abbreviations used for materials in the pit, the pottery types Thick Interior Cordmarked and Thin Interior Cordmarked, which appear to have very different temporal positions in stratigraphic levels of the site, are lumped together and called ICM, meaning Interior Cordmarked. There are other examples of this sort of treatment.

The greater bulk of the report of Dr. Cross is concerned with artifact typology. As far as I am able to determine, the pottery types are set up so





that they are time-markers and are not based only on differences in form or manufacturing technique. This section on the typology has only one marring feature in it. This is Table 17, where the materials from the one "pure" Early Woodland component from the lowest humus in the Lowlands are left out because Dr. Cross thought there were too few sherds to bother to enumerate. This prevents one from understanding exactly what the ceramic complex of her Early Woodland cultural entity is.

The description and typology of the artifacts other than pottery seem to be done in a rather different manner. First of all, it seems that the types of projectile points, scrapers, and other artifacts are not determined wholly, or even partially, on their temporal and spatial significance. In many cases Cross mentions that the so-called sub-types are better time-markers than her types. In her charts she gives the distribution of types but, unfortunately, the sub-types are not mentioned. With many of these types, although there is a description of them and pictures in the back of the book, there is no cross-referencing, nor in the descriptions are the dimensions given. Quite frankly, the whole report would have been greatly improved if the sort of typology for projectile points that Suhm and Krieger used in their Handbook of Texas Archaeology had been followed. From the picture one gets of the artifacts other than pottery, there appears to be great continuity in her sequence. Some of this, because of the peculiar way she has done the typology, may be more apparent than real.

My final discussion concerns the Summary and Conclusions. One finds in this section that Cross believes she has a Paleo-Indian horizon at Abbott Farm. This is based upon three parts of fluted points, not Folsom points, two of which were actually found above pottery. Neither in her section





summarizing the cultural context nor her cultural reconstructions is there any definite proof of Paleo-Indians having lived here; there certainly is nothing that can be considered a Paleo-Indian component. She believes that the next horizon belongs to the Archaic period but again, in her excavations, she records no actual horizon or pit that has Archaic materials as such. In light of not being able to define an Archaic component, she bases her Archaic horizon on the occurrence at the Abbott Farm of artifact types found in other Archaic horizons in the east, though she readily admits that Archaic types seem to persist somewhat longer in the ceramic horizons in New Jersey than elsewhere. Quite frankly, her presentation of the evidence for an Archaic occupation is extremely poor. I do believe, however, that a re-analysis of some of the materials from her pits might indicate that certain of them do belong to one or more Archaic components. For example, Pits 55, 23, 27 and 77 from Excavation 2, Pits 5, 6, 7 and 11 from Excavation 3, and Pits 4, 22, 23, and 32 from Excavation 9, as well as a number of the caches, are without ceramics, and some of them certainly have artifacts which are similar to those found in Archaic horizons elsewhere. Perhaps these are actually components of the Archaic at Abbott Farm. If so, the matter is not demonstrated in Cross' conclusions.

Humus 4, from the Lowlands, Pits 22 and 85 from Excavation 2, Pit 18 from Excavation 3, and Pits 11 and 12 from Excavation 9, have specific pottery types and artifacts that could be classified as Early Woodland. Using these materials combined with ones out of stratigraphic context that are considered Early Woodland, on the basis of comparisons with materials from other areas in the eastern United States, this period is defined. I think it would be clearer if she treated these two kinds of evidence separately.





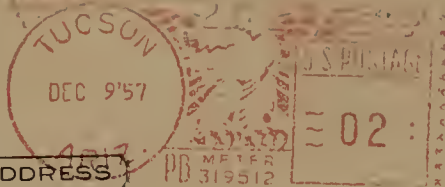
The Middle Woodland definition is in better shape as it is based upon materials from Humus 3 from the Lowlands and from pits or levels from the Bluffs above which contain similar materials. The definition of the Late Woodland horizon, based mainly on those materials found in Layer 2 from the Lowlands also seems just.

All in all, despite what I consider to be flaws in analysis and interpretation, we should be thankful for having this site report from New Jersey, from which we have very few archaeological treatises.

Richard S. MacNeish,  
National Museum,  
Ottawa, Ontario.



University of Arizona  
TUCSON 25, ARIZONA



THIS SIDE OF CARD IS FOR ADDRESS.

Dr. Richard S. MacNeish  
Human History Branch  
National Museum of Canada  
Ottawa  
Canada

Human History Branch

XXXXXXXXXXXXX  
XXXXXX

Ottawa, December 17, 1957.

Dr. Raymond H. Thompson,  
Department of Anthropology,  
University of Arizona,  
Tucson 25, Arizona.

Dear Ray:

Enclosed please find two reprints on cave materials from Mexico and a larger one published by the American Philosophical Society on some Formative remains. I am pleased to announce to you that a 600-page manuscript on the archaeological sequences in the Sierra de Tamaulipas is now in the hands of the printers of the American Philosophical Society. (Thank God.) Actually the best summary on sequences in all my work for northeastern Mexico is in my article on settlement patterns in the Viking Fund Volume, No. 23. Unfortunately, Sol Tax and Sherry Washburn fouled me up and I never received any reprints.

There a couple more articles coming out on the Tamaulipas stuff--one on beans, and the other on Bat Cave Type corn--and I'll put you on my reprint list for when these are published.

Sincerely yours,

Richard S. MacNeish

Enclosures

a.

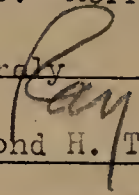


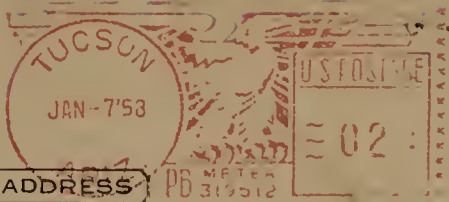
UNIVERSITY OF ARIZONA

( x ) Department of Anthropology    Dec 8, 1957  
(   ) Arizona State Museum

This is to acknowledge with thanks receipt of your fine review of the NJ Archaeol vol. It will be submitted for publication in the April issue. Do you have any material on your Mexican work in reprint form still available. If so, I would appreciate something on the Tamaulipas caves, etc. See you in Chicago. Merry Xmas.

Sincerely

  
Raymond H. Thompson



THIS SIDE OF CARD IS FOR ADDRESS

Dr. Ricahrd S. MacNeish  
Human History Branch  
National Museum of Canada  
Ottawa  
Canada



THE UNIVERSITY OF ARIZONA  
TUCSON

COLLEGE OF LIBERAL ARTS  
DEPARTMENT OF ANTHROPOLOGY

Feb 28, 1960

Dr. Richard S. MacNeish  
National Museum of Canada  
Ottawa, Ontario

Dear Scotty:

Some time ago now Doh Collier sent me your rejoinder to Walt Taylor's review of your Sierra de Tamaulipas report. Both he and I agreed that it should be published, but toned down a bit in spots. I promised to write to you about it. As you see, it has taken me some time to do so.

It will appear in the April 1960 issue (which you can gather is behind schedule) in the Facts and Comments section. I have polished up a couple of spots editorially and written an abstract, and have added the bilbiographic details. The only part which has been seriously changed is the last page which I retyped. I enclose your original page with my scribbled corrections so that you can see what I have done. I hope that you will have no objection. I have already sent the MS to the printer. You will see the whole thing in galley proof. The title is simply "Rejoinder to Taylor."

Best regards,

Raymond H. Thompson  
Editor  
Society for American Archaeology

Enclosure

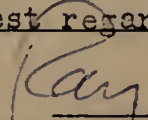
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✓  
UNIVERSITY OF ARIZONA

( x ) Department of Anthropology Jan 6, 1958  
( ) Arizona State Museum

This is to acknowledge with thanks receipt of APS monograph on the Panuco site, Mangelsdorf, MacNeish, and Galinat on maize, and Whitaker, Cutler, and MacNeish on the Ocampo cucurbits. Thanks also for adding me to your reprint list for the forthcoming papers on beans and corn.  
~~It was nice to chat even so briefly with you in Chicago.~~

Best regards

  
Raymond H. Thompson



Third

Finally, Taylor believes that my cultural units might "more realistically and significantly be grouped into three units". <sup>as much as</sup> Since on page 166 <sup>167</sup>, table 22, of my report, <sup>tentative</sup> there is a similar classification, <sup>on</sup>

Taylor is <sup>either</sup> not very original, <sup>or</sup> he did not read the report very carefully.

~~However~~, I think the small units or subdivisions are a more realistic and significant grouping at present for a number of reasons. First of all it is

more practical to analyse starting with <sup>an</sup> artifact, <sup>the</sup> artifact assemblage,

then phase, and then later, finally arriving at larger groupings such as

traditions when more complete data <sup>is</sup> available. Secondly, "the publication <sup>Taylor (1948:127) has already pointed out that</sup>

and descriptions of the small units serve to acquaint.... colleagues of the

findings, to elicit from them the antithetic or corroborative information

urgently required, and to provide the general literature with comparative

material with which to lend depth and significance to other finds from

other sites". (Taylor, 1948, pp. 127). <sup>Finally,</sup> Thirdly, I do not see the merit

<sup>and as</sup> of Taylor's classification <sup>that</sup> lumps Lerma with Alamagre when there are

only six general types held in common out of 43 and when the two have

entirely different <sup>modes of</sup> subsistence (hunting vs. agricultural) and community

patterns (nomadic microbands vs. semi-sedentary <sup>a</sup> micro-bands).

In conclusion, I am appalled that Taylor, who <sup>se</sup> has set himself up <sup>reputation as</sup>

<sup>a skillful and influential</sup> as infallible destructive critic of New Archaeological endeavours wrote

the review he did. <sup>World</sup> This is even more disconcerting since he <sup>is well established,</sup>

~~to have~~ spent considerable time studying the report. I sincerely hope

that he will show <sup>archaeological</sup> more ability to analyze ~~archaeological~~ materials in his

long-awaited Coahuila <sup>monograph</sup> magnum opus than he has revealed in his study of <sup>review of</sup>

<sup>my Tamaulipas report</sup> materials in my monograph.

Richard S. MacNeish  
National Museum of Canada.





November 14, 1960.

Dr. R. H. Thompson,  
Department of Anthropology,  
University of Arizona,  
Tucson, Arizona.

Dear Ray:

I hear by the grape vine that Walt Taylor has a reply to the rejoinder by MacNeish of Taylor's book review. Since I will be leaving for field work in a month or so, if my NSF grant comes through, I was wondering if I could see a type-written copy of it now so that I can make a reply, to a reply, to a rejoinder, to a book review, to a book, or perhaps I should better say a reply, to a reply, to a reply, to a reply, to a reply.

Yours sincerely,

/cp

Richard S. MacNeish.







# THE UNIVERSITY OF ARIZONA

T U C S O N

COLLEGE OF LIBERAL ARTS

DEPARTMENT OF ANTHROPOLOGY

Jan 8, 1961

Dr. Richard S. MacNeish  
Human History Branch  
National Museum of Canada  
Ottawa, Ontario

Dear Scotty:

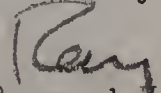
Sorry to take so long in answering your note of Nov 14. the delay is proportional to the lag in the appearance of AMERICAN ANTIQUITY. I enclose a copy of the galley proof of Walt's most recent.

I hope that you will agree with me after reading it--and I gather from the tone of your description of the reply to a reply, etc. etc. in your letter,--that you are in a mood to agree with me--that the time has come to call a halt to this series. I cannot see the value in putting \$20 a page into something that should go by 4-cent stamps between you and Walt. I send you this galley for information, therefore, rather than to supply you with ammunition for another reply.

If you still feel that you have been badly treated, there is a better way to publicly defend yourself. After all, you wrote a book which has to stand on its own. You run the risk of a bad review as does any author. You have already had a chance to say that you think the reviewer was not fair and that he missed the point. Instead of beating the Tamaulipas report to death in the process of making a theoretical point, why not sit down and prepare an essay on the theory and practice of defining phases. The Tamaulipas materials could be used as examples along with stuff from other areas and other sites. Something like that would have a concrete value because it would be a positive contribution. Another reply would be just another negative statement.

I hope your NSF grant came through. Happy new year.

Sincerely,

  
Raymond H. Thompson  
Editor, SAA

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## REPLY TO MACNEISH

WALTER W. TAYLOR

## ABSTRACT

In a rejoinder to Taylor's review of his Tamaulipas monograph, MacNeish restated his method of analysis and reviewed the system he used to establish his phase sequence. Taylor claims that MacNeish has defended his position by introducing new data and by presenting a point of view not expressed in the original monograph. Taylor insists that MacNeish's reply only confirms his original criticism that the Tamaulipas phase sequence is built on the premise that one excavation unit equals one phase.

I DO NOT BELIEVE that a scientific journal is a place to make *ad hominem* remarks—or to reply to them. Therefore, I do not intend to deal with certain of the statements and implications contained in MacNeish's *Rejoinder* (1960) to my review (Taylor 1960) of his Tamaulipas report (MacNeish 1958). On the other hand, I do believe that a scientist has the right, even the obligation, to defend his statements, particularly against charges which seem to be based on maltreatment of the evidence. On this premise, then, I should like to discuss the major points raised by MacNeish, in order to set the record straight and to vindicate my "ability to analyze archaeological materials," upon which he has cast his doubt. This will be an analytic study in its own right, not merely and solely a rebuttal. As such, I hope it may prove both informative and illuminating.

MacNeish rejects my contention that one of his major premises was that "if it is one deposit according to his own way of thinking, then it is one phase, even one band, even one occupation," that is, a "one excavation unit— one cultural unit" premise. Now, I fully expected that he might not recognize this premise. In fact, I actually said that I believe it to be "perhaps unconscious." Thus, it is no surprise that it does not appear in his original monograph explicitly stated in so many words. But it also follows that any refutation of it must lie, not in demonstrating the absence of such an explicit statement or in noting the presence of other stated premises, but rather in careful and penetrating analysis of his theoretical scheme, both explicit and implicit, and of his procedures in handling empirical data and inferences derived therefrom.

In my study for reviewing his monograph, I did this. I do not believe that MacNeish has done it before writing his *Rejoinder*. In fact, he says that he will "repeat," "reiterate" his premises and bases for establishing phases, as if by doing so his case would stand proved. But I said in my review, and I say here, that only by going "deeper into his procedures and his premises" can we (and this includes MacNeish himself) understand his theoretical position. And I believe even more strongly today that this is the only way MacNeish can prove himself right and me wrong. Restatement of explicit premises does not establish or refute unconscious premises.

Be this as it may, the most significant thing about his *Rejoinder* is the amount and character of new data introduced and the startling differences between these, substantively and in implication, from those that he gave in the original publication which I reviewed. Aside from the questionable propriety of belaboring me for failing to analyze data not in the publication under review, the issue which I shall engage here concerns these differences and their significance for an understanding of MacNeish's theoretical position and his use of archaeological materials.

In the *Rejoinder*, he says:

I shall briefly *repeat* what my premises actually were . . . . As indicated on page 9 of the Tamaulipas work, the major premise is that a distinctive congerie [sic] composed of an adequate sample of artifacts and traits that occurs at one or more (pure) components, each of which represents an occupation, shall be the basis for establishing a phase [italics mine].

But these words do not appear on page 9, or on any other page, of the original monograph! Therefore, he is not "repeating," as he says he is doing. Referring to page 9 of the Tamaulipas work, we find that what he actually said was:

The complexes, congeries, or clusters [of artifact types], belonging to these various levels or layers of excavated sites, are here called components, while complexes of types from unexcavated sites (surface collections) are merely called site collections. From the standpoint of description and organization, it was necessary to classify these components with identical or very similar congeries of types in convenient units. Such a unit I have called a "phase". . . . I have assumed that a phase represents the preserved material remains of a single group of people at what, for heuristic purposes, is considered a moment in time. . . .

That is all there is on the establishment of phases: nothing about "distinctive congeries," or "adequate sample," or "pure component." Furthermore, he says that he "assumes" the phase represents a single people, not that a (single) occupation constitutes one criterion for a phase.

Thus, it is hardly a wonder that MacNeish says, I "did not discern this major premise." It was not mentioned in the report I was reviewing and, left to my own devices, I certainly did not get even an inkling of it while studying the monograph itself, because everything he said there led me to infer quite another premise, namely the "one congeries—one phase" premise which I stated in my review. In that place, I gave considerable documentation for my contention, and I see no advantage in repeating or amplifying it here, particularly since MacNeish has not rebutted any of that documentation in his *Rejoinder*. But let us continue with this matter of the establishment of phases.

MacNeish says in his *rejoinder*:

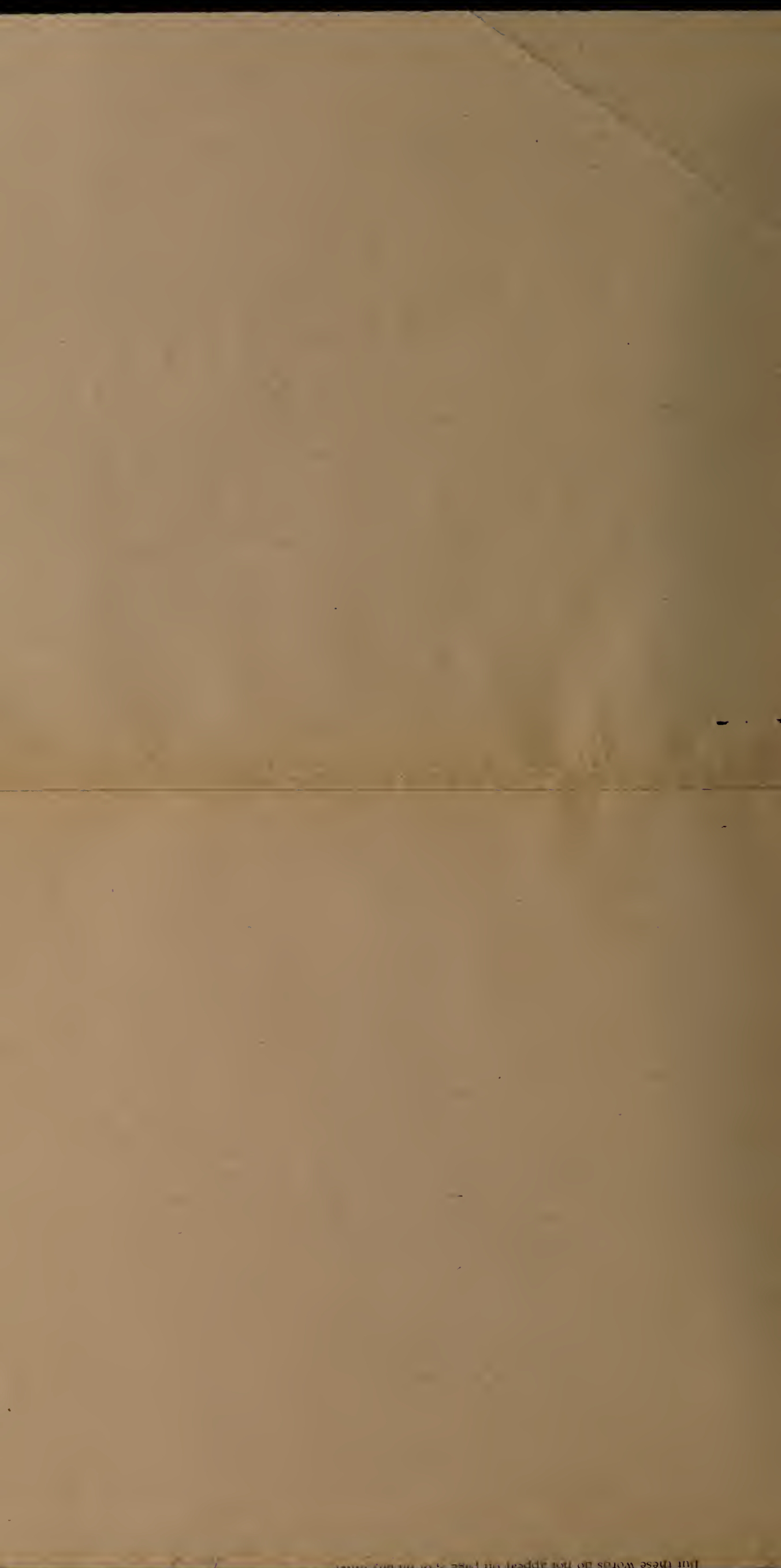
Now let me *reiterate* briefly how and on what basis the phases were established [italics mine].

He then proceeds to give a long exposition of a sequence of analysis by which his phases allegedly were constructed. This exposition is interesting, informative, and valuable. But to my knowledge, *this is the first time it has appeared in print!* Certainly it is not to be found in the Tamaulipas report—if it had been there, my review would have been quite different. On page 152 of that report, there is a presentation of his sequence of analysis, in what I take to be chronological order as evidenced by such phrases as "then," "from these studies," "at this stage of our analysis." The only trouble is that this account does not jibe with that which he gives in his *Rejoinder*. In the latter publication, MacNeish says that he is appalled because I wrote the review I did. I can tell him now that I wrote the review I did because I was reviewing his original monograph, not his *Rejoinder*.

Yet he uses the word *reiterate*. What can this mean? Webster says the word means "to repeat; to say or do over again or repeatedly." Is MacNeish trying to give the readers of his *Rejoinder* the impression that he is repeating statements which I failed to catch in his original report? Surely he could not be doing this, because this would be untrue: he is not repeating. Nor can he be talking to those "careful and understanding" readers of the original report, whom he asks to stand by while he refutes me, because they would certainly know that he is not repeating. For whom, then, is he writing? Perhaps he is writing for himself. Perhaps he is trying to convince himself that his Tamaulipas work was indeed based on the most solid and acceptable premises of his *Rejoinder*, not on those of his original report. This is just a suggestion.

Now let us look at another aspect of his statements on the phases. No less than eight times in the course of the relatively short *Rejoinder*, he uses the phrase "pure component" to describe the nature of those particular artifactual congeries or clusters to which he says he gives the name *phase*. In one place he says:

[Taylor] made this mistake [of not recognizing the major premise which MacNeish claims to have used] in spite of the fact that I used the word *phase* as opposed to complex, *pure* components in distinction to possible, probable, and doubtful components or site collections; and I made a point of not recognizing certain phases until pure components occurred . . . [italics his].





But in the original monograph, the phrase *pure component* is never used and, as far as I can see, even the concept, however worded, is not used except in two very specific and non-theoretical descriptions where he refers to a "pure" site (p. 7) and to "relatively pure examples of the Lerma Phase" (p. 30). It is very obvious, therefore, that the concept of "pure component" as a criterion of phase is a product of MacNeish's later years. While, in the Tamaulipas report, it is not even mentioned in the sections dedicated to theory and method (pp. 5-10) or even in the discussion of the phases themselves (pp. 152-65), in the *Rejoinder* it plays a prominent, even dominant, part in his (new) theoretical scheme. I am very sorry he did not define it and use it in the monograph which I reviewed, because I am sure that it would have been basic to my understanding of his theoretical point of view. However, since he has never defined it, even in the *Rejoinder*, perhaps it is not a clear concept in his mind and had better be left for some still later time for definitive treatment.

Furthermore, there is only one place in the original monograph (p. 9) where he explicitly distinguishes between the concept of phase and that of complex:

A second classificatory term is "complex" which I apply to a congeries or cluster of types that occurs in a number of site collections (non-excavated sites). . . . In reality my complexes are tentative phases.

Nothing is said here about "pure components" versus "impure" complexes, and the term *complex* is specifically restricted to site collections, that is, congeries from unexcavated sites. The fact that MacNeish does not conform to his own definitions, as in his use of the word *complex* for components of excavated sites (p. 152) and for artifact assemblages in general in his section entitled "Summary of the Artifact Complexes" (p. 152), does not alter the fact that according to his own theoretical dictamen the concept *complex* applies only to groups of artifact types from unexcavated sites. It is perhaps significant, and in support of the major premise which I suggested in my review, that MacNeish, when writing in a strictly theoretical vein, makes no provision for complexes (tentative phases) in excavated sites. If my analysis of his premises is correct, his theoretical reasoning might go something like this: the physical grouping of artifacts (the component) is given by the mechanics of digging and the only cultural decisions that have to be made concern the further grouping of components into phases; in non-excavated sites, on the other hand, the initial component is not given physically, except in the broadest and hence virtually meaningless sense, thus cultural decisions have to be made on two levels, both component and phase; this creates more uncertainty and "tentativeness" and brings the need for a different term (*complex*) to designate such congeries. If this is correct, it supports my contention that the "one excavation unit—one cultural unit" premise is central to MacNeish's theoretical scheme. It is further significant that, as one might expect, he does not (cannot?) adhere strictly to this distinction.

To bring up another point, I note that in his *Rejoinder*, he says:

This will further illustrate that . . . the master sequence [of phases] definitely was not "based largely on the findings in Diablo Cave" [as Taylor claimed in his review].

This came as a definite surprise to me, because in his original report he had said:

In the analysis, the artifacts from the six layers from Tm c 81 [Diablo Cave] were *studied first*. Five sequential complexes of artifacts, or components of phases, were noted; from early to late they were tentatively named Diablo, Lerma, Nogales, Eslabones, and Los Angeles [p. 152; italics mine].

Since there were but nine cultural units defined from this region and since he specifically says that five of them had been *studied first and noted* in the analysis of artifacts from Diablo Cave, I thought myself on pretty safe ground in making the statement I did. What he said about his ceramic analysis led me to the same conclusion, although the pottery from Diablo Cave was apparently in the lot that was studied after the first two sites, Tm c 86 and Tm r 79, had been analyzed. Nevertheless, the analysis of Diablo Cave was certainly among the first in ceramics and the very first in other artifacts.

But then the thought struck me that perhaps MacNeish's phases were not established on the basis of artifactual or of ceramic analysis, but on some other information and *before* these analyses were undertaken. I began to search the Tamaulipas report for evidence which might throw light on this possibility. Some of what I found I published as documentation in my review, but it was not until the *Rejoinder* appeared that there was explicit evidence from his own words.

In trying to refute my contention (that his sequence of phases had been "based largely on the findings in Diablo Cave"), he said in his *Rejoinder*:

. . . three phases — originally called periods of the Pueblito culture — were established in 1946, before I had ever heard of Diablo Cave.

In the first place, if only three had been established before hearing of the site, my contention would still be correct because he says that five had been noted and tentatively named from the analysis of Diablo Cave. Thus the sequence would still be "largely" from Diablo Cave.

However, let us look at MacNeish's research time-table for a moment. He says that his first work in the Sierra de Tamaulipas began in April, 1946, and that it was primarily one of reconnaissance (p. 6). By May 26th, of that same year, the first field season was over, and he was able to study other collections of Tamaulipean material. He says (p. 7):

Thus, by May of 1946 archaeological materials were available from 171 sites in Tamaulipas. To establish, in a preliminary way, what artifact types existed and what the artifact complexes were, and to determine which sites were most suitable for excavation, an analysis was then made. . . . This analysis, which started in 1946, continued in 1947. . . . Thus in November of 1948, excavations began [italics mine].

Somewhat later in the same publication (p. 21), he says:

I first visited the rock shelter [Diablo Cave] in the spring of 1946 while making preliminary survey of the northern Sierra de Tamaulipas. At that time Pedro Lerma (guide), Roberto Pavon (assistant) and I sank a test pit. . . .

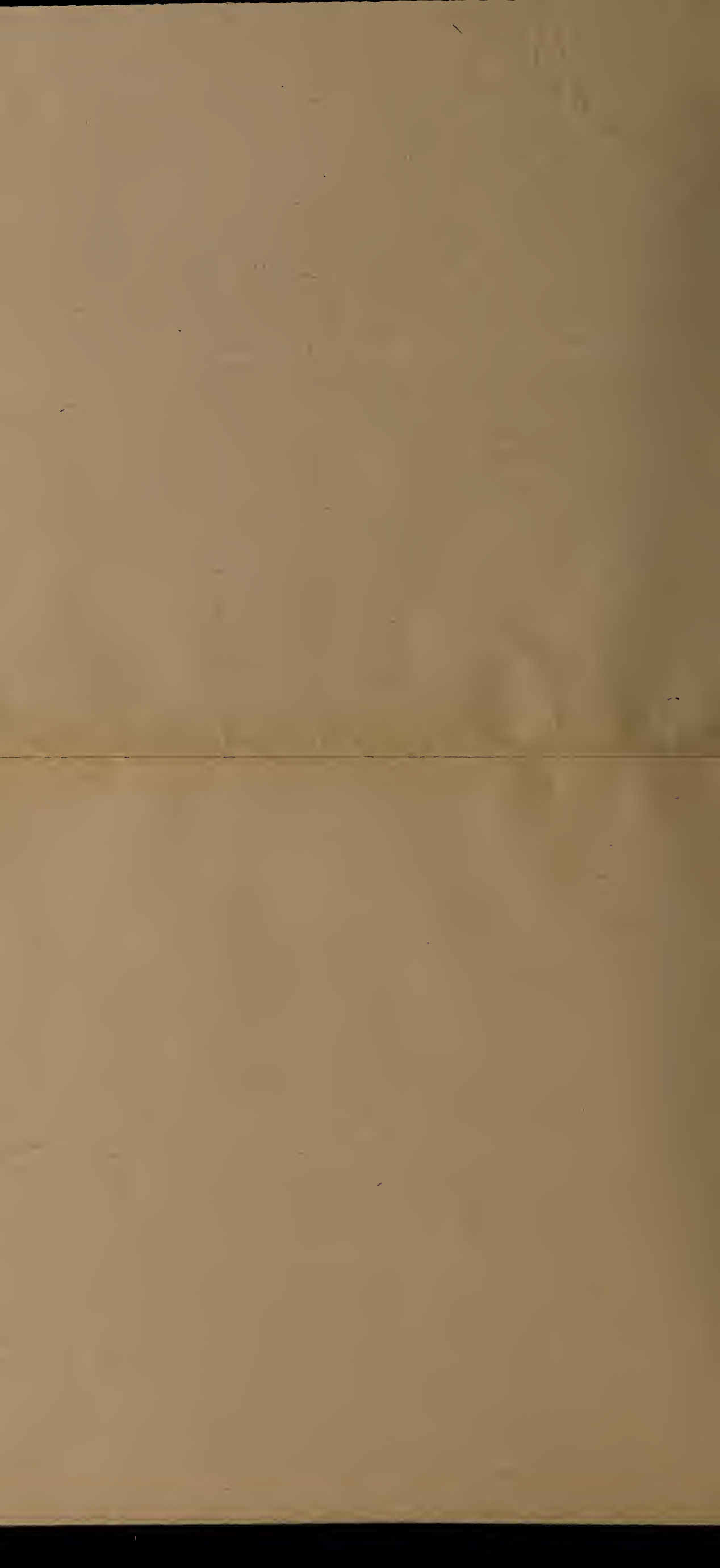
These facts and statements when put together yield the following time-table:

1. field survey of Sierra de Tamaulipas begins in April, 1946
2. three phases established before Diablo Cave "heard of"
3. Diablo Cave visited and tested before end of field survey
4. field survey ends May 26th, 1946, after 13 sites visited and tested (his first published report, MacNeish 1947, says only 8)
5. study of other collections of Tamaulipas material after survey ends
6. preliminary analysis to establish artifact types and complexes
7. excavations begin in November, 1948

I find it interesting that by Step 2 in the above time-table, even before "preliminary analysis" had been made, MacNeish had been able to establish three phases — not complexes, but *phases*. I believe that this illustrates clearly that his statement about his premises and criteria for the establishment of phases, as quoted above from the *Rejoinder*, is just not true. In this instance, I question very seriously that by the time of Step 2 of his research he could have been able to determine that his congeries were "distinctive" or what really constituted a "pure" component. As for the criterion of "adequate sample" for the determination of phases, I offer examples of his statements on the two sites upon which rests the determination of the Eslabones phase (one of the "first three" established):

[In Tm r 86] a trench was put on one of the small plazas, and the top soil was removed from two house platforms. These gave us only a small sample of materials; more would be very desirable [p. 41; italics mine]. [in Tm r 6] the refuse was not deep, and the materials were sparse [p. 52; italics mine].





## 58 — ANTIQUITY.....

Even more significant, perhaps, is the change in evaluation which came over MacNeish when he wrote his *Rejoinder*, in which he says:

The Eslabones phase was established on the basis of *abundant materials* . . . found first in Level 3 (a house floor) at Tm r 86 and duplicated in two test pits in house-platform floors at Tm r 6 [*italics mine*].

Such differences between the two publications are not uncommon, but the really significant thing is, I believe, that the statements in the *Rejoinder* support the theoretical position which MacNeish assumes in that publication, while the statements found in the original report support the more general, the less precise and demanding criteria set forth there. I refer my present readers to the use of the qualificatory words such as "tentative," "probable," and "possible" when MacNeish refers to phase assignments in the *Rejoinder* — and to the consistent lack of same in the assignments of his original monograph. For example, in the former he says:

A primary corollary to this [his "major premise," as quoted in the first quotation above] is that congeries of artifacts which *came* from excavation units may *tentatively* and with definite qualifications be assigned to one of the established phases on the basis of limited similarities. A second corollary is that in cases where there are no established phases . . . components or site collections may be tentatively classified as belonging to complexes [*italics his*].

But nowhere in his original monograph, and I repeat NOWHERE, even during his discussion of the cultural components of his excavated sites (pp. 21-45) and in his "Summary of the Artifact Complexes" (pp. 152-65), does he use in his phase assignments the word *tentative* or *tentatively* or their conceptual equivalents. I find only one place in the entire monograph where *tentatively* is used in this manner: on page 105 in his description of pottery.

Only in his accounts of the "surveyed sites" (pp. 45-57) does he use qualifying words and phrases at all, and even with these one cannot be sure that there will be concordance between what he says there and what he says in his *Rejoinder*. For examples, I refer the interested reader to sites Tm c 322 and TM r 323 which, in the latter publications, are "considered *probable* components [*italics his*]" and which "were given this less definite status because the materials came from a context that did not indicate [*italics mine*] a definite, single occupation." Compare the above with the following from the original report:

[the sherd types] indicate that [Tm c 322] belongs to the La Salta Phase [p. 54; *italics mine*].

[the combination of pottery types] indicated that Tm r 323 belongs to the La Salta Phase [p. 54; *italics mine*].

What happened to the "probable," "less definite" status? How is it that in his original publication the materials "indicate," while in his later publication the same materials do "not indicate?" If these were isolated instances, they might be attributed to a simple misstatement. But when there are so many of them and when the positive assignments of the original report are so consistently followed by qualified assignments of the same components in the *Rejoinder*, happenstance appears to be ruled out.

From all the above, it seems very clear to me that MacNeish did indeed set up phases, at least three of them and probably more, on the basis of excavation units — before detailed and thorough cultural analysis and comparison had been made. It is also clear that he identified artifactual congeries determined by excavation unit, that is, level or layer, as cultural components which he grouped with identical or very similar congeries into phases. Of course, the gimmick here lies in the words *identical* or *very similar*. Since definitions of such terms are largely a matter of opinion and were not defined by MacNeish and since space here is limited and my review contained statements as to my own opinion on this subject, I shall not elaborate upon this point. But using his own statement from the original publication (p. 152 and quoted above), I still maintain that his sequence of phases is *largely* (that is, five units out of nine) based upon Diablo Cave as I said in my review that it was. Finally, it is very evident that MacNeish has changed both his theoretical position and his statements concerning many of the facts between the time he wrote the original monograph and the time he wrote the *Rejoinder*.

I do not see much use in discussing the rest of MacNeish's points. They are largely matters of opinion or interpretation and would require an unjustifiable amount of space to discuss. However, two things I do wish to acknowledge. First, if, as he says, I did not answer a plea for information from him, or from any colleague, I am truly sorry. Frankly, I do not remember getting the two letters he says he wrote, and a check in my files fails to produce them. However, I apologize for my share in this particular fiasco. In the second place, I should like to point out that the cultural groupings which I suggested as possibly being more realistic and significant were indeed taken directly from his monograph. I intended to make no claim of originality, but on reading over what I wrote, I can see how this interpretation might be made. I apologize for this also and assure MacNeish that I did not mean to plagiarize, but rather to support, something which he had so plainly proposed.

MACNEISH, R. S.

1947. A Preliminary Report on Coastal Tamaulipas, Mexico. *American Antiquity*, Vol. 13, No. 1, pp. 1-15. Menasha.

1958. Preliminary Archaeological Investigations in the Sierra de Tamaulipas, Mexico. *Transactions of the American Philosophical Society*, n.s., Vol. 48, Pt. 6. Philadelphia.

1960. *Rejoinder* to Taylor. *American Antiquity*, Vol. 25, No. 4, pp. 591-3. Salt Lake City.

TAYLOR, W. W.

1960. Review of "Preliminary Archaeological Investigations in the Sierra de Tamaulipas, Mexico," by R. S. MacNeish. *American Antiquity*, Vol. 25, No. 3, pp. 434-6. Salt Lake City.

SOUTHERN ILLINOIS UNIVERSITY

Carbondale, Ill.

August, 1960





CANADA

PLEASE QUOTE

FILE.....

NATIONAL MUSEUM  
OF CANADA

DEPARTMENT  
OF  
NORTHERN AFFAIRS AND NATIONAL RESOURCES

HUMAN HISTORY  
BRANCH

January 11, 1961.

Dr. Raymond H. Thompson,  
Department of Anthropology,  
College of Liberal Arts,  
University of Arizona,  
Tucson, Arizona.

Dear Dr. Thompson:

Your letter and the galley proof arrived after  
Dr. MacNeish had left for his field work in Mexico.

Do you wish me to hold them for his attention  
when he returns to Ottawa on June 1, or would you prefer to  
have it returned to you for the present?

Sincerely yours,

Carolyn Provost, Secretary,  
Division of Archaeology.

/cp

*Thank you - I suggest holding it for his return*

*RHS*

*1/17/61*





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Dr. Raymond H. Thompson,  
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Division of Archaeology.

/cp

1871

Received of the  
Hon. Secy of the Navy  
the sum of \$100.00  
for the purchase of  
the ship "Albatross"

for the purchase of the ship "Albatross"

for the purchase of the ship "Albatross"

for the purchase of the ship "Albatross"

for the purchase of the ship "Albatross"



THE AMERICAN MUSEUM OF NATURAL HISTORY  
CENTRAL PARK WEST AT 79th STREET  
NEW YORK 24, N. Y.

## DEPARTMENT OF ANTHROPOLOGY

H. L. SHAPIRO, Ph.D., Chairman  
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LOUIS DUPREE, Ph.D., Research Associate  
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ANTOINETTE K. GORDON, Associate  
EDGAR M. QUEENY, A.B., Field Associate

January 16, 1961

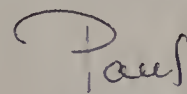
Dr. Richard S. MacNeish  
Canadian Museum of Human History  
Ottawa, Ontario  
Canada

Dear Scotty:

I have recently been commissioned to write the article on the utilitarian artifacts of Central Mexico for the Handbook of Middle American Indians. As a result, I would find it most convenient to have both your Huasteca and Tamaulipas reports within easy reach, since they will probably have a lot to do with my typology and will certainly be essential for comparative purposes. Would you be willing to donate to the cause?

(I think I sent you a copy of my Valley of Mexico report -- if not, please let me know and I will remedy the situation which, in that case, would certainly be lop-sided).

Sincerely yours,



Paul Tolstoy



January 19, 1961.

Mr. Paul Tolstoy,  
Dept. of Anthropology,  
American Museum of Natural History,  
Central Park W. at 79th Street,  
New York 24, N. Y.

Dear Mr. Tolstoy:

I am sorry to say that Dr. MacNeish left two weeks ago for his field work in Mexico. He will not return to Ottawa until June 1.

So far as I know Dr. MacNeish does not have extra copies of his two reports you mention,,but if you drop him a note c/o Hotel Penafiel, Tehuacan, Puebla, Mexico, he can let me know and I, if possible, will forward them to you.

You can also ask him about your Valley of Mexico report.

Yours sincerely,

Carolyn Provost, Secretary,  
Archaeology Section.

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Ottawa, January 22, 1951.

Dr. A. M. Tozzer,  
Peabody Museum,  
Harvard University,  
Cambridge 38, Mass.

Dear Dr. Tozzer:

For the last few months I have been endeavouring to prepare a report on my excavations of "Archaic" sites in the Tampico-Panuco, Veracruz. In my analysis I have discovered that some of my figurines from my early levels are extremely similar to those found in the Maya area, particularly those from the Mamon phase at Uaxactun. I have looked at illustrations in various reports and have corresponded and exchanged photographs with R. E. Smith but I still have not really seen any figurines from these early levels.

I would be extremely interested to know if you have in the Peabody Museum collections any pre-classic Maya figurines. I would appreciate your informing me if you have such available, and I will endeavour to make a trip to the Peabody Museum in the near future.

Sincerely yours,

Dr. Richard S. MacNeish  
National Museum  
Ottawa, Canada

a.



Ottawa, March 10, 1951.

Dr. Alfred M. Tozzer,  
7 Bryant Street,  
Cambridge 38, Mass.

Dear Dr. Tozzer:

On March 15, 16th, and 17th, I will have the opportunity to visit Peabody Museum at Harvard. I would very much like to see the figurines you wrote to me about from my area and further south. If there are definite similarities in some of the figurines to those I found from Panuco, I would like to photograph them at that time and, with your permission, to use these materials in my report.

During the early part of the week, I will be at the American Museum of Natural History and can be reached by writing in care of Dr. Gordon Ekholm.

Sincerely yours,

Dr. Richard S. MacNeish  
National Museum  
Ottawa, Canada

a.





Ottawa, May 12, 1958.

Mr. Rudy C. Troike,  
Department of Anthropology,  
University of Texas,  
Austin, Texas.

Dear Mr. Troike:

Enclosed please find three reprints that may be of some help to you. One is on the early sequence at Panuco, Vera Cruz, another is an initial survey of Tamaulipas, while the third is a reprint on stuff we did in the southwest part of Tamaulipas. Unfortunately, I have no reprints of the other four articles I have written on Tamaulipas. However, just be patient and my magnum opus on the Sierra de Tamaulipas will be printed by the American Philosophical Society (this year, I hope).

Sincerely yours,

Richard S. MacNeish

Enclosures

R.S.M./a.



THE UNIVERSITY OF TEXAS  
AUSTIN 12

DEPARTMENT OF ANTHROPOLOGY

May 19, 1958

Dr. Richard S. MacNeish  
National Museum of Canada  
Ottawa, Ontario  
Canada

Dear Dr. Macneish:

Your three reprints were received today, and I wish to express my very greatest appreciation for them. I would also like to thank William Taylor for communicating my request to you. I have had a close interest in your work for a number of years, since I am a native of Brownsville and have specialized in the Northeastern Mexico-Texas region.

This summer I am to conduct the fieldwork for the U.C.L.A. expedition to the Sierra de Tamaulipas, so that your work acquires added importance to me. It is very good news that your full report is to be published soon. I read the manuscript which you sent to Alex Krieger in 1954, and was considerably impressed by it. More recently I have looked over the copy of your dissertation on file with the I.N.A.H. in Mexico. The work should be a very fundamental contribution to the archeology of a highly significant region, and one which is of especial interest to me. We are looking forward to the expedition this summer. Last year we did work in central San Luis Potosi, in some Huastec IV sites.

Again let me express my thanks for your reprints. We will be looking forward to the appearance of your new monograph. With very best regards,

Yours truly,

*Rudy C. Troike*  
Rudy C. Troike









Ottawa, December 22nd, 1949.

Mr. Turner,  
South Nahanni Trading Post,  
c/o Fort Liard,  
Northwest Territories.

Dear Mr. Turner:

I am an archaeologist working for The National Museum of Canada. During the past summer, I visited Fort Simpson on the Mackenzie River, and this coming summer shall excavate at that Post. I also hope to get over to South Nahanni and would like very much to investigate the caves just up the river from you, and also would like to fly in to the caves above the falls, which reputedly have pottery, and so forth, in them.

I would appreciate your telling me what bush pilots or individuals know of these caves further up the Nahanni. Also I would like your opinion as to whether there are old Indian materials, particularly pottery, in these caves. I also believe you have a boat at South Nahanni, and know the river a few miles north of your Post, and I wondered if you might guide me up into that territory next summer.

Wishing you a Merry Christmas  
and a Happy New Year,

Sincerely yours,

a.

Dr. Richard S. MacNeish.





Ottawa, February 23, 1951.

Mr. Dick Turner,  
Turner's Trading Post,  
Junction of the Liard and Netla Rivers,  
Northwest Territories.

Dear Dick:

This winter I have had a couple of chances to meet your brother at Carleton College and have had some interesting talks with him. He informs me that you set up a post at Trout Lake last fall. Will the trail to your Post be useful in the summer time? As you may remember I am rather interested in getting out to Trout Lake as I have heard of archaeological sites on the north end.

My wife will be travelling with me in the north during the early part of the summer. She will be on a fellowship to make an anthropological and socialological study of a small Sladé community. In talking over our plans, we have thought that your band on the Netla River, since it is relatively independent of white man's direct influence, might be worth looking over with an eye to making a study of the village. Therefore, on or about July the 1st, we may be visiting you at your Post. When our plans become more settled, we will write you again and tell you of them.

I hope that Vera and the children had an enjoyable visit when they went outside last



Mr. Turner,

Ottawa, February 23, 1951.

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fall. By the way, do you have two or your children in school outside this winter?

Let me know if there is anything I can get for you outside, either here in Ottawa or in the States, that could be of use to you up in the north.

Sincerely yours,

Dr. Richard S. MacNeish  
National Museum  
Ottawa, Canada

a.





CORRESPONDENCE

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